

both these conditions are fulfilled that perfectly sharp images of spectral lines extending from the red to the high zinc line 2138.30 can be photographed on the same surface. Celluloid films are used, glass not being sufficiently flexible.

Using the middle position showing the whole spectrum on a plate, the angle is 40° , and the curvature is 190 mm. radius.

The condensers are of quartz, and are plano-cylindrical—one being double the focus of the other. The object of this, when spark-spectra are being photographed, is to concentrate on the slit a line instead of a point of light, as would be the case if ordinary lenses were used.

When photographing phosphorescent spectra—or, in fact, any spectra the wave-lengths of which are either unknown or require verification—I always photograph on the same film a standard spectrum, usually of an alloy of equal molecular weights of zinc, cadmium, tin, and mercury. This forms a hard somewhat malleable alloy, giving throughout the whole photographic region lines the wave-lengths of which are well known. The chief objection to this alloy is its volatility—the poles requiring frequent adjustment. Recently I have used pure iron for this purpose; this has the advantages of giving a great number of fine lines whose wave-lengths are accurately known, and not being very volatile, the poles do not rapidly wear away. If the poles are kept about 1 mm. apart there is little or no interference from air lines.

The most simple method of applying the standard lines to an unknown spectrum is by the successive employment of two slightly overlapping diaphragms immediately behind the slit, one being used for the experimental and the other for the standard spectrum. In this way, without disturbing the instrument, the two spectra can be recorded on the plate one over the other; the overlap of 1 mm. being in the optical centre of the train. The resulting negative is then transferred to a micrometer measuring machine of special construction, having a screw of $1/100$ of an inch pitch, and a means of accurately determining $1/1000$ of its revolution—thus measuring directly to the hundred-thousandth of an inch. In this way, in a five-prism spectrograph having lenses 700 mm. focus, it is possible to determine wave-lengths of photographed lines to the sixth figure.

“Experimental Contributions to the Theory of Heredity. A. Telegony.” By J. C. EWART, M.D., F.R.S., University of Edinburgh. Received May 29,—Read June 1, 1899.

I. *Introductory.*

The belief in telegony, or what used to be known as the “infection of the germ” or “throwing back” to a previous sire, has long prevailed

It may for all we know be as old as the belief in "mental impressions," which has had its adherents since at least the time of the patriarchs. During the eighteenth century the "infection" doctrine was frequently discussed by physiologists, and since Lord Morton, in 1820, addressed a letter to the Royal Society on the subject, believers in "infection" have been increasing all over the world, with the result that one seldom now hears of breeders or fanciers who are not influenced by the doctrine, while physicians and others interested in the problems of heredity either as a rule take telegony for granted or see nothing improbable in the "infection" hypothesis.

It must, however, be admitted that, notwithstanding the criticisms of Weismann and others, very different views are entertained by the believers in telegony, not only as to the cause, but as to the results, of "infection." By some telegony is confounded with simple reversion or atavism, while the better informed generally assume that "infection" invariably results in the subsequent offspring repeating more or less accurately the characters of the first or of a previous sire. In a breeders' journal of some standing there appeared recently under the heading "Colour of Animals" the following sentence:—"Greys show in breeding a great tenacity of assertion, as they are few in comparison to other colours in the Stud Book, but they reappear and no doubt go back to the Arab, and prove telegony to be a fact."* This shows simple reversion is sometimes mistaken for telegony. In support of the view that "infection" is commonly supposed to lead to "throwing back" to a previous sire many instances could be given, but the following from an article on telegony by De Varigny will suffice. De Varigny states that an ordinary cat which had kittens to a tailless Manx cat subsequently produced several tailless kittens to a normal cat of her own breed.†

An extended series of experiments with various kinds of animals has led me to the conclusion that if there is such a thing as telegony, it is more likely to result in the subsequent offspring "throwing back" to an ancestor of the "infected" dam than to a previous mate. This view of telegony (which has not been insisted on hitherto) will be made at once evident by an example. A sable collie crossed with a Dalmatian produced three pups which in their coloration are extremely like young foxhounds; instead of numerous small spots each has a few large blotches. According to the common view of telegony this collie, if infected, should next produce with a dog of her own breed one or more Dalmatian-like pups. If, however, the offspring of a collie and a Dalmatian are like foxhounds, the subsequent offspring to a collie of the same colour and strain could hardly be expected to present Dalmatian characters, *i.e.*, show numerous small spots. But if "infection" as a

* 'Live Stock Journal,' May 12, 1899, p. 588.

† 'Journal des Débats,' September 9, 1897.

rule results in the subsequent offspring "throwing back" either to the ancestors of the sire or the dam, it will be extremely difficult, if not in many cases impossible, to distinguish telegony from simple reversion.*

But though "infection," if it does take place, is likely, as a rule, to lead the subsequent offspring to resemble the ancestors of the dam, it may in certain cases possibly lead to their "throwing back" to a previous sire. This result might follow if the previous sire happened to be highly prepotent. For example, Highland heifers often produce to a Galloway bull hornless black offspring indistinguishable from pure Galloways. If infected by the Galloway bull, these heifers might afterwards produce Galloway-like calves when mated with long-horned bright coloured bulls of their own breed.

It is now commonly believed that if there is such a thing as telegony it results from the unused germ cells of the first (or previous) sire infecting—blending with—the unripe germ cells in the ovaries of the dam. Were this possible, the subsequent progeny would in all probability in a mild way resemble the previous sire, but if this is impossible, then infection—due perhaps to some obscure change in the constitution or reproductive system of the dam—is more likely to lead to more or less marked reversion to the ancestors of the dam. All my observations point to its being impossible in the Equidæ for the unused male germ cells of the first sire to infect the unripe ova. The spermatozoa lodged in the upper dilated part of the oviduct of the mare are dead, and in process of disintegrating, eight days after insemination; they probably lose their fertilising power in four or five days. There is no reason for supposing that in the Equidæ they survive longer in or around the ovary. Further, though at the time of fertilisation there may be several large Graafian follicles in each ovary containing maturing ova, all these follicles disappear long before the period of gestation is completed. The subsequent foals are developed from successive new crops of ova into the composition of which it is inconceivable any of the spermatozoa of the first sire could by any chance enter. A study of the ovaries hence tends to confirm the view that "infection" (if there is such a thing) is as likely to cause reversion to a former ancestor of the dam as a "throwing back" to a previous sire.

Having made these general observations, it will be well next to consider critically the case of "infection" communicated in the letter to the President of the Royal Society in 1820 by the Earl of Morton.

* That reversion ever occurs has been questioned by Bateson ('Materials for the Study of Variation') and others, but I have already ('Nature,' February 9, 1899) proved beyond doubt that reversion can be easily induced by intercrossing distinct types, and I have recently heard of several instances of spontaneous reversion—reversion not induced by intercrossing.

Though many other instances of supposed "infection" have been recorded, Lord Morton's mare may be said to still hold the field—the theory of telegony still mainly rests on the assumption that this historic mare was "infected" by a quagga some years before she passed into the hands of Sir Gore Ouseley and produced three "colts" to a black Arabian horse. One might even go further and without much exaggeration assert that the telegony hypothesis at the present moment mainly rests on an allegation by Sir Gore Ouseley's stud groom.

It has been generally assumed that Lord Morton's mare (a nearly purely bred chestnut Arab) was "infected" for two reasons (1) because the subsequent offspring were of a yellowish-brown colour and more or less striped, and (2) because, according to Sir Gore Ouseley's stud groom, the mane of one of the striped foals had always been upright, while in another it arched to one side clear of the neck. The presence of stripes in the subsequent offspring has never been questioned, nor yet is there any doubt that when Lord Morton in 1820 inspected the "colts" the mane in the filly was upright as in the quagga, while that of the colt resembled the mane of Lord Morton's quagga hybrid. There is, however, an absence of reliable evidence that the filly's mane had *always* been upright as alleged to Lord Morton by Sir Gore Ouseley's stud groom.

Were the evidence in support of this allegation satisfactory, there would I think be no escape from the conclusion that Lord Morton's mare was "infected" by the quagga. Hitherto the presence of stripes on the "colts" has generally been looked upon as affording strong evidence of "infection." Believers in telegony admit that stripes are not uncommon in Norwegian and certain other breeds of horses, but, with Mr. Darwin, they have taken for granted that they never or very rarely occur in Arabs.

I find, however, that though in Arabia dun-coloured horses are disliked and never used for breeding, stripes even in the most renowned strains are not so uncommon as is generally supposed. I have now a purely bred Arab filly of about the same colour as Lord Morton's filly, but, unlike the filly we have heard so much of, both the fore and hind legs are marked with distinct dark bars, and there are faint indications of stripes across the withers and a distinct dorsal band. The history of this filly (bred by Mr. Wilfred Seawen Blunt at Crabbet Park, Sussex, and very kindly presented to me) is well known for many generations; none of her ancestors could possibly have been "infected" by a zebra. The dun colour and stripes are doubtless the result of simple spontaneous reversion, for, unlike Lord Morton's mare, there is no history of a cross in her pedigree. This filly proves that even in high-caste Arabs of the best desert blood a dun colour and stripes may unexpectedly appear.

As to the occurrence of stripes in other breeds I could give, were it necessary, many instances. A year ago I had in my possession a light bay (or yellow dun) pony, which showed nearly as many stripes on the trunk as the Gore-Ouseley filly, and in addition had several interrupted narrow stripes on the forehead.* Moreover, the stripes on the Gore-Ouseley "colts," while agreeing with stripes occasionally seen in horses, differ in their arrangement from the stripes in the quagga. The stripes themselves are evidence of reversion, but nothing more, and seeing that pure bred horses sometimes show quite as many stripes, we are not justified in assuming that but for the dam of the "colts" having been first mated with a quagga the stripes would not have appeared.

Hence unless it is proved that the mane in the filly and colt were naturally erect, or nearly erect, the case for the "infection" of Lord Morton's mare will be lost. It may be well to quote the passage from Lord Morton's letter referring to the mane. It is as follows:—"That of the filly is short, stiff, and upright, and Sir Gore Ouseley's stud groom alleged it never was otherwise. That of the colt is long, but so stiff as to arch upwards and to hang clear of the neck, in which circumstance it resembles that of the hybrid. This is the more remarkable as the manes of the Arabian breed hang lank and closer to the neck than that of most others."†

I am not prepared to accept the allegation as to the manes for the following reasons:—

1. I have had twelve zebra hybrids under observation, and in each case the mane, though erect to start with, always after a time arched over to one or both sides. The stud groom's statement, it seems to me, proves too much. If in the quagga hybrid and in all my horse hybrids the mane, sooner or later, falls to one side, it is a little remarkable that in the pure bred two-year-old filly it had been always upright.

I may here mention that the hair of the mane of zebra hybrids is shed annually; it is for this reason that the mane in hybrids is never long enough to hang close to the neck.

2. The mane in the drawing of the filly by Agassé is not represented as upright, but as lying to one side. If the mane had remained erect during the first two years, by virtue of shedding its hairs, it could not very well have lost this habit and fallen completely over to one side subsequently, say, during the fourth year. From the mane being erect in 1820, and hanging to one side in 1821 or 1822, when Agassé's drawing was made, the presumption is that the mane of the "colts" had been cut some time before they were examined by Lord Morton.

Two years ago I had a bay Arab with a mane which was to start

* See Fig. 36, 'The Penycuik Experiments,' A. and C. Black, 1899.

† 'Phil. Trans.,' 1821.

with short, stiff, and upright, some months later it arched freely to one side, as in my zebra hybrids, and later still it hung lank and close to the neck.

3. There is always an intimate relation in the Equidæ between the mane and the tail, when the mane is short and erect the upper third or so of the tail is only covered with short hairs, which, like the hairs of the mane, are annually shed. Lord Morton noticed nothing peculiar about the tail of the "colts," and the tail of both the colt and filly in Agassé's drawings is the tail of a high-caste Arab. This seems to me to warrant the conclusion that the filly's mane had been hogged some time before Lord Morton's visit.

It thus appears that the evidence in support of the belief that Lord Morton's mare was "infected" by the quagga is at the best far from satisfactory. The same may be said of the evidence in support of all the other supposed cases of telegony in the Equidæ—of, amongst others, Lord Mostyn's mare, referred to by Darwin;* of the mule-like mare in the Paris Gardens, referred to by Tegetmeier and Sutherland;† and of the African ass (*Equus asinus*), still in the Zoological Gardens (London), which now and then has a reddish-coloured foal, like the cross-bred foal she produced in 1883 to an Asiatic ass (*E. hemionus*).

Although I am now satisfied that Lord Morton's case throws little light on the telegony hypothesis, like many others I had no very decided views on the subject some years ago, and hence when arranging in 1894 to make a collection of horse embryos, I decided to repeat, as far as circumstances permitted, what is commonly called Lord Morton's experiment. For this purpose, I procured early in 1895 three zebras and a number of mares. Two of the zebras died during the winter of 1895, but the third—a handsome stallion of the Chapman variety (*E. burchelli* v. *chapmani*) still survives and is now thoroughly acclimatised.

During 1895 I only succeeded in mating the zebra with one mare, and hence there was only one hybrid born in 1896. During the last two years however, quite a number of hybrids have made their appearance, and the dams of several of the hybrids have subsequently produced pure-bred foals. The time has hence come, when some of the results of the experiments may with propriety be communicated to the Royal Society.

"II. Experiments with West Highland Ponies." By LORD ARTHUR CECIL, Orchardmains, Kent, and J. C. EWART.

The first mare mated with the zebra was a black, West Highland pony (Mulatto), set apart for the telegony experiments by Lord

* 'Animals and Plants,' vol. 1, p. 435, 1875.

† 'Horses, Asses, and Zebras,' p. 81.

Arthur Cecil. The better bred West Highland ponies are supposed to have descended from "Armada" horses, and are hence perhaps related to Mexican and Argentine horses, so often dun-coloured and partially striped. Mulatto's hybrid (Romulus, born 12th August, 1896) is on the whole more a zebra than a pony both mentally and physically. He is especially remarkable in being more profusely striped than his sire (the zebra Matopo), in having a heavy semi-erect mane, which is shed annually, and in having a mule-like tail, from the upper third of which the longer hairs are periodically shed. The body colour of the hybrid varies from a dark orange colour to a mouse dun; the stripes of a reddish-brown colour on the head are dark brown or nearly black on the trunk and limbs.

In the number and plan of the stripes, the hybrid agrees more closely with the Somali zebra than with any of the Burchell zebras. Over the brow, *e.g.*, there are narrow rounded arches instead of somewhat broad pointed arches as in his sire, the neck and trunk have quite double the number of stripes found in the sire, while over the croup in the position of the "gridiron" of the mountain zebra there were at birth irregular rows of spots which in course of time blended to form somewhat zig-zag, narrow, transverse bands. The ears are nearly as large as in the sire, while the eye-lashes are longer and distinctly curved. In his movements the hybrid resembles his sire, and like his sire he is always on the alert, very active and suspicious of unfamiliar objects. Further in his call he agrees far more with his sire than his dam. In being profusely striped, Romulus differs greatly from the quagga hybrid bred by Lord Morton, in which the stripes were fewer in number than in many dun-coloured horses.

Mulatto's second foal arrived in July, 1897, the sire, Benazrek, being a high-caste grey Arab horse. Like Lord Morton's colts, Mulatto's foal by the Arab horse, in make, action, and temperament, agreed with ordinary foals, but it differed from the majority of foals in presenting quite a number of *indistinct* stripes—subtle markings only visible in certain lights. These stripes differed but little from the body colour, which varied from dark bay to brown. Though few references have been made to the occurrence of stripes in foals, they are, we find, far from uncommon. As is well known, Mr. Darwin once bred a striped foal by putting a cross-bred bay mare to a thoroughbred horse. This foal was for a time marked nearly all over with obscure dark narrow stripes, plainest on the forehead, but also distinct over the croup.*

There is no figure of Mr. Darwin's striped foal, but from the description given, there can be little doubt that the markings were more abundant than in Mulatto's second foal. In this foal (as in Mr. Darwin's) the stripes became more and more indistinct, and by November

* 'Animals and Plants,' vol. 1, p. 60.

they had almost vanished. Unfortunately, the foal died when about five months old, and hence it is impossible to say whether any of the stripes would have persisted. It will be evident that Mulatto's second foal helped but little to clear up the vexed "infection" problem. Mulatto missed having a foal in 1898, but she recently produced at Knole, Kent, her third foal. The sire (Loch Corrie) of this foal belongs to the Island of Rum section of the West Highland ponies, and closely resembles Mulatto. The third foal has about as many stripes as the second. As in the second, they are most distinct over the croup and hind quarters; and as in the second, they differ both from the markings in the previous sire, the zebra, and from the markings of the hybrid Romulus.

This third foal, which was born on the 6th of May, 1899, seemed like the second, to lend some support to the "infection" hypothesis. Fortunately, since it made its appearance, two other West Highland mares have had foals to Loch Corrie. These foals put all doubt as to the nature and significance of the stripes on Mulatto's second and third foals at an end.

One of the dams is of a brown colour, the other is nearly black. Though neither the brown dam nor the black has ever seen a zebra, both foals are marked in very much the same way as Mulatto's, and some of the stripes in one of the new foals look more like persisting than the stripes on Mulatto's third foal. Hence, in order to account for the markings on Mulatto's foal to the grey Arab, and on her foal to the black West Highland pony, it is unnecessary to fall back on the "infection" hypothesis.

"III. Experiments with Shetland, Iceland, Irish, Thoroughbred, and other Ponies." By J. C. EWART.

An effort was made to cross four Shetland ponies with the zebra stallion, but I only succeeded in obtaining one hybrid. The dam (Nora) of this hybrid closely resembles, except in size, the Island of Rum ponies—she is a small edition of Mulatto. Her first foal, by a black Shetland pony, was of a dun colour and nearly as striped as Sir Gore Ouseley's filly; her second is the most zebra-like of all my hybrids; her third closely resembles her sire, a bay Welsh pony. For some time after birth there were faint indications of stripes over the hind quarters of this foal, but now it is a year old there are no markings or any other suggestions of a zebra. It is not a little suggestive that the foal bred from this pony before she was mated with the zebra was distinctly striped, while the subsequent pure bred foal has no stripes.

Of five Iceland ponies put to the zebra only one produced a hybrid. This hybrid was faintly striped, and showed less of the zebra than any

of the others. The dam, a prepotent yellow and white (skewbald) pony, had first of all a light bay foal to an Iceland pony. Her third foal, by a bay Shetland stallion, is a skewbald, and in the size and arrangement of the brown patches closely resembles the dam. There is no hint whatever that the Iceland pony has been "infected" by the zebra.

Several Irish mares were put to the zebra, and two of them (bays) have first produced hybrids and subsequently pure bred foals. A cream coloured Irish-Canadian mare unfortunately died before her hybrid foal was born. One of the bay mares had a bay hybrid richly striped; the other a hybrid with but indistinct stripes. The subsequent foals—one by a chestnut thoroughbred horse (Tupgill), the other by a hackney pony (Mars Royal)—are bays, not only devoid of stripes but affording no indication whatever that their dams had been previously mated with a zebra.

Although I experimented with seven English thoroughbred mares and an Arab mare, I only succeeded with one, a small chestnut. This mare produced twin hybrids last summer; yesterday she had a foal to a thoroughbred chestnut horse (Lockstitch). One of the twins died soon after birth, the other, richly but unobtrusively striped, in its colour and make strongly suggests its dam. The chestnut mare's new foal neither in make, colour, nor action in any way resembles a young zebra nor a zebra hybrid. In 1897 a bay mare by a bay Arab horse (Hadeed) was for some months in foal to the zebra. Since she miscarried in 1896 she has had two foals to a thoroughbred horse (Lockstitch). Neither of these foals in any way suggests a zebra. In this case the unused germ cells of the zebra had presumably a better chance of reaching the ovum from which the first of the two pure-bred foals was developed than is usually the case.

Attempts were made to cross Welsh, Exmoor, New Forest, Norwegian, and Highland ponies with the zebra without success, and though a cross-bred Clydesdale has twice had a hybrid, she has not yet produced a pure bred foal. The experiments as far as they have gone afford no evidence in support of the telegony hypothesis.